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ON THE LOGICAL APPLICATIONS OF  
PHYSIOLOGY TO PATHOLOGY;

AN

INTRODUCTORY LECTURE

TO THE COURSE ON

THE PRINCIPLES AND PRACTICE OF MEDICINE,

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TO  
NEIL ARNOTT, M.D.



## A LECTURE.

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GENTLEMEN,—On the occasion of appearing before you for the first time, as your instructor in the Theory and Practice of Medicine, it would seem not only natural, but almost imperative, that I should explain to you the views which I entertain of the just and appropriate basis of that theory and that practice. It would, for several reasons, have been well, could I have entered fully into an explanation of the kind; and, forced as I am by limitation of time to relinquish the idea of even an attempt at a complete exposition of the subject, I will still endeavour, at least, to touch upon some fair share of its more prominent points. It will be thus practicable for me to give you, inferentially, an insight into the character of the evidence, and into the mode of using that evidence (especially when derived from certain collateral sciences) which I am disposed to believe essential to the solid establishment of the Science of Pathology, or General Doctrine of Disease.

§ II. Placed conspicuously before you are four Tablets, on which are inscribed the fundamental plans, by which, in the sequence adopted, the science of Pathology is, I conceive, to be *directly and immediately formed*.

### I.

CASES OF DISEASE SUBMITTED TO  
OBSERVATION,  
LEAD TO THE ESTABLISHMENT OF  
*Individual Pathological Phenomena.*

### II.

INDIVIDUAL PATHOLOGICAL PHENOMENA SUBMITTED TO  
INTERPRETATION,  
LEAD TO THE ESTABLISHMENT (PROVISIONALLY) OF THE  
*Nexuses of those Phenomena.*

### III.

THESE PROVISIONALLY ESTABLISHED NEXUSES, SUBMITTED TO  
NUMERICAL COMPARISON,  
ARE JUDGED, AND SO LEAD TO THE ESTABLISHMENT OF THE  
*General Laws of Diseases.*

## IV.

THE GENERAL LAWS OF DISEASES SUBMITTED TO  
CLASSIFICATION,  
CONSTITUTE

*The Science of Pathology.*

Hence the intellectual processes by which the Science would be formed, are successively, Observation, Interpretation, Numerical Comparison, and Classification. And, commencing with individual cases of disease, we should be gradually led to classified laws,—the substance and essence of the science of disease, and the highest generalizations at which human intelligences may legitimately grasp. I need, in truth, scarcely remind you, that in our Science, First Principles are utterly unattainable; for the counterpart of the gravitation of the Natural Philosopher, the Pathologist may sigh in vain.

Now, the prominent characteristic of the scheme for the foundation of Pathology I here set forth and espouse, is its independence. It recognises facts of its own kind—not only kindred, but identical, in quality—as its sole basis. It makes no provision for the qualification of its laws by, much less for the formation of these laws out of, facts or generalizations belonging to other sciences, be the consanguinity of these sciences ever so close, and, *ex natura rerum*, direct. It holds Pathology to be a code of doctrines, founded upon distinct, proper, and special elements—elements whose ultimate adaptation to that code takes place utterly irrespective of *à priori* notions pressed forward by cognate branches of knowledge. It gives Pathology a place as purely self-dependent and distinct, in regard of the means of its *actual establishment as a science* (but of this only), as is held by chemistry, for example, in the sciences of matter. Or, more clearly to particularize, this scheme refuses by implication to recognise as the *true formative material* of the science of Pathology (that is, the sum of classified laws of diseased actions), inferences deducible, prior to experience, from current notions (whether possible, probable, likely, very likely, or certain), held concerning the natural texture and healthy actions of the frame. This scheme denies that

Physiology (vital, chemical, or physical) is the basis of Pathology, in the sense that acquaintance with the one secures by involution acquaintance with the other. This scheme denies that Physiology is the basis of Pathology, in the sense that, given the recognised healthy life of an organ, the consequences of the derangements of that life can by any forms of reasoning, inductive, deductive, analogical, or other, be positively predicated prior to actual experience of their character and habitudes. It affirms, on the contrary, that from the Observation, Interpretation, Numerical Comparison, and Classification of those derangements themselves (collated, of course, with healthy conditions), are their nature and laws alone to be established.

But is not this scheme tainted with heterodoxy? To all seeming it must plead guilty to the charge; it does, in truth, clash with the doctrine *verbally accepted* as part of medical grammar, and repeated from lip to lip, as though constituting a fundamental judgment, against which appeal was neither desirable nor possible. But the habitual repetition of that doctrine does not prove that it accords with the serious conviction of thinking men; for, as a close analyst of human character has aptly observed, "there are certain things which all the world goes on repeating, simply because they have been once said;" (a) and it may be that the apophthegm, "All Pathology is based on Physiology,"—become a household phrase in medical circles—is precisely a proposition of the kind. Now I believe that it actually is a proposition of the kind; and that, if its signification were closely canvassed, its soundness would be generally demurred to. Let me proceed to justify this view of its character.

§ III. There seem to be three possible ways in which Physiology may be conceived to hold scientific relationship to Pathology. Physiology may be imagined to provide the means of establishing *à priori*, otherwise of predicting, the conditions of Pathology; or it may be supposed capable of explaining pathological relationships, when evolved; or, lastly, it may be considered as supplying a standard of com-

(a) Montesquieu.



parison, and as suggestive of plans and measures, observant or experimental, for investigating pathological conditions.

(a) As regards the first of these pretensions, the prophetic, I have already intimated my belief in the powerlessness of Physiology. It must, in truth, be admitted that, by its agency alone, we can never advance beyond the simple demonstration of the primary truism, that if healthy action be interrupted, morbid action necessarily ensues, (just as, it might be affirmed, that a watch with a damaged spring will not work, as when that spring was in order;) but the what, the when, the how, the how long, the what for, and the where to, of the new morbid actions, are all matters belonging to the province of experience alone. Let us begin, by way of illustration, with the very simplest Pathological phenomena—those in which mechanical function is predominant. Take the instance of a wound in the gall-bladder. We know from Physiology that the gall-bladder is meant to contain bile; and we can certainly logically predict, that if the containing-bag be suddenly pierced, (b), the contained fluid will escape more or less abundantly. But beyond that, our powers of positive prophecy fail; we could not, except as the result of clinical experience, in any wise or in any degree affirm, what the results of the effusion of the bile might be. Take, again, the instance of an aged person, the neck of whose femur suddenly snaps: Physiology might certainly justify us in the positive affirmation, that inability to use the limb for progression would follow the accident; it might by diligent consideration of the precise lines of action of the various connected muscles (admitting this knowledge to be perfect in its way) make a correct assertion, prior to experience, as to the unnatural direction the limb would assume. But all this is merely mechanical; it is the sort of inferential power which would indeed prove Physiology to be the formative basis of Pathology, were man a mere machine. But he is not simply a machine; his femur cannot be broken without vital action being disordered; and the sum total of physiological knowledge could never have established, prior

(b) If perforation be *slowly* effected, it is almost needless to observe, that even this limited prophetic power is lost.



to actual experience, the vital consequences, local and general, of that simple injury. No; scarcely, I affirm, could it have supplied even a solitary link of the great chain of impressions which, originating in the disruption of a few minute nerves, vessels, and bone-lamellæ, may eventually make themselves felt in every fibre and every function of the frame. Again; Physiology would enable us to affirm, prior to actual experience, that if all entry of air into the lungs were suddenly arrested mechanically, death must almost instantaneously ensue. Vital reactions have no opportunity for complicating the problem. But make the obstruction less complete,—make death less instantaneous, and less purely mechanical,—give time for vital force to come into play, and what comes of the physiological gift of prophecy? What Physiology could, prior to experience, have taught us the functional differences that ensue, where œdema of the glottis, laryngitis simple or diphtheritic, phthisical, cancerous, or syphilitic, extrinsic pressure from solid tumour or from aneurism, are severally the causes of mechanical obstruction? (c)

No; once vital action steps in, Physiology is posed. For, to take the most favourable example for the exercise of its faculty in regard of such action,—that of a simple vital property, say, muscular irritability;—let it be granted that Physiology, having thoroughly analysed this property in regard of the quality of its natural stimuli and the phenomena and immediate effects of its healthy action, is in a position to foretell absolutely the direct and essential influences on itself of its exaltation, impairment or perversion; still for all practical purposes of pathological and clinical prophecy, the power will prove abortive and valueless. For in nature muscular irritability is not a condition that suffers alone,—that property is changed in connection with others; in connection with innervation, too, and structure; and hence the effects of its perversion vary from those which (on the admission made)

(c) Illustrations may similarly be taken from the varying effects of sudden mechanical section, and slowly-produced breach of continuity, of nerves, and of the spinal cord, &c.

could have been anticipated. The clinical co-existences of the perversion are as far beyond the prophetic ken of Physiology, as though this knew naught of the simple property. Besides, the admission made is, in reality, one which the Pathologist is not called on to make. On the contrary, the conditions of muscular irritability have not been fully explored by Physiology, if trust may be placed in recent pathological inquiries. Physiology, in truth, had not prepared us for the singular circumstance (established, to all appearance, satisfactorily, by numerous and careful experiments on man and the lower animals in a state of disease), that in certain morbid conditions the voluntary muscles will respond to, and act under, the influence of the will, while they refuse to obey the ordinary extrinsic stimuli,—while they remain motionless under the passage of a galvanic current, for example, though the conducting power of the muscular tissue has undergone no impairment. (d) Turn from a vital property to a simple function,—local innervation. The finer nervous filaments are so constantly accompanied by blood-vessels, that it is maintained the nerve and vessel are physiologically but one;—the integrity of the vessel and its contents is essential to the action of the nerve; unless blood flow in healthy proximity and abundance, beside the nerve filament, the action of this is at an end. Yet, look upon that woman, pale, marble-like, inanimate. Naturally possessed of ordinary powers of hearing, she has just chanced to lose some pounds of blood by hæmorrhage,—her life is in imminent danger,—her vessels seem drained of their contents,—a filiform pulse remains alone, to show that a dwindled current still trickles through her artery; yet her hearing is hyper-acute. She, who heard simply like other people, when her blood and nerves stood in natural relationship, is now distinctly, and almost painfully, alive to the slightest whisper at the further end of a large room, when her frame is quasi-bloodless. Could Physiology have predicted this condition of the auditory nerves? Could

(d) It will be seen that reference is made to the recent experiments of M. Duchenne.

Physiology have foretold the mysterious influence that saves those nerves from impairment of functional power, (the brain is even anæmiated, for the sufferer is in a constant state of semi-syncope,) amid the general wreck of the vital fluid?

Consider next those varied combinations of altered texture and impaired function, known clinically as morbid processes and diseases. Does the mantle of the prophet belong more rightfully to Physiology in regard of these, than of the simpler phenomena we have just referred to? Think not of the absurdity which seems to lie on the very face of the question, —absurdity because that, which fails in regard of elementary phenomena, can scarcely be expected to succeed, when these are complex in infinite variety and sequence: think not of this, but appeal to the decree of experience. Is there, then, in the whole range of diseases, general, diathetic, and local, one solitary instance whose phenomena and whose pathogeny, from first to last, might have been foretold prior to experience, by any existing, or any fairly-conceivable knowledge of Physiology of texture or function? That no such malady exists, from the simplest inflammation to the most complex blood-disease, is so completely a matter of everyday observation, that to illustrate the fact by examples would be an absolute waste of time. Yet, in spite of this, men have occasionally been found to act as if the process were natural and easy of execution. Men have actually ventured to fashion (and give names to) diseases by grouping together morbid phenomena in such union, and in such sequence, as, from physiological considerations, they are led to imagine those phenomena *ought* to arise. In such manner did Cullen produce his description of “synocha,” or pure *idiopathic* inflammatory fever, a disease which he acknowledged he had never seen.(e) Yet (such is the weight of authority in Medicine) notwithstanding this acknowledgment; notwithstanding the admission of Cullen’s successor, Dr. James Gregory, that during thirty years he, too, had never met with synocha; notwithstanding the analogous statement

(e) *Vide* Bartlett, on the Fevers of the United States (Pref. p, x. Ed. 2).

of Dr. T. Bateman, and notwithstanding that clinical experience still looks in vain for the disease, various systematic writers continue to describe this creature of the imagination as an observed and observable reality!

But, further than all this, physiological *à priori* Pathology, finds idolaters who yield it still higher powers,—who assign it the function of announcing, prior to experience, the conditions and effects of various infractions of hygienic rules. Physiology, in other words, has been, and is perpetually made to teach (what is to the tenth power, perhaps, more difficult) prophetic *Ætiology*. An example or two will show with what success. Not many years ago a Committee sat in the House of Commons for the purpose of inquiring into the effects of factory labour on adult, and especially on infant health. Medical men were examined in numbers,—men of zeal, of honour, of accomplishment. But they chanced to be physiological *ætiologists*; they were men who denied the necessity for seeing the localities, watching the mode of working, scrutinizing the habits of the workers, and, above all, substantiating the actual condition of the nutrition and innervation—the life—of those, concerning the influences of whose occupation on health they were required solemnly to pronounce. Idle toil! They had philosophy and physiological dogmata at command; they knew much about oxygen and carbonic acid, imperfect oxidation of the blood, floating flue, and bronchial irritation, late hours, over-work, imperfect hæmotosis and mal-nutrition. And, armed with such philosophy and such physiology, they went to work. The results are recorded; and what are they? Why the system of man is scarcely exposed to an ailment, that was not by one or other of the physiological *ætiologists* announced as an inevitable effect of the pursuit of the species of labour in question. Let me not weary your patience by rehearsing the series. It will answer our purpose to refer to three kinds of affection concerning the necessary frightful prevalence of which all were agreed; namely, scrofula, deformity of the limbs, and flattening of the plantar arch. The sentence went forth, and was accepted for the time. But by-and-by men



who had actually occasionally seen factory children, and had not noticed the universality of the visitations announced, thought it might be as well to inspect a large body of them carefully, with a special view to the detection of scrofula, deformity of the limbs, and flattening of the plantar arch. Dr. Holme, for instance, (a name which should never be heard within these walls without a feeling of the deepest and most respectful gratitude) examined 401 factory children, and found eight only of them scrofulous. Mr. Harrison passed 1,653 in review, without finding a single example of deformed limbs; and Sir David Barry inspected the feet of 111 girls, without stumbling on a single flattened plantar arch. It was absolutely found that the factory population was freer from some of the evils, they had been so lavishly accommodated with by the prophetic ætiologists, than the population of the surrounding towns at large.

Let me offer you another illustration. Ramazzini (sound though he generally was in his judgments) occasionally sinned logically by describing, on mere *à priori* physiological grounds, the influences of certain trades on health. He erred thus, for instance, in regard of tobacco-workers; and Cadet, Patissier, Percy, Mérat, and many others, followed in his train. Now among the effects ascribed by these writers to the occupation referred to, appear habitual headach, vertigo, nausea, sneezing, vomiting, colic, and affections of the lungs (asthmatical and other), tremors, narcotism, bloody flux, discolouration of the skin, emaciation, and actual death. Reading these portentous statements (and I have positively put them below the actual mark), might not one suspect that an indictment for homicide fairly lay against men who habitually indulged in the use of snuff and tobacco?—Might not one marvel how the existence of these luxuries, as cheap purchaseable commodities, was to be explained? Parent-Duchâtelet arose, and solved the mystery. He examined, either personally or through competent unprejudiced agents, 4,513 men employed in the tobacco-manufactories of France; and he found, simply, that these men did *not* suffer from nervous ailments, *nor* from chest diseases, *nor* from abdominal

complaints, *nor* from emaciation, *nor* from discolouration of the skin, more than the community at large; that, further, their ordinary duration of life was not below the usual average—nay, that the dismissal of workmen *in consequence of advanced age* was a frequent occurrence! Ramazzini and his copyists forgot altogether (that which nothing but experience can teach the precise influence and effect of in any particular instance) the great *law of accommodation or habit*. (f)

(b). Secondly; the pretensions of Physiology to explain and connect the phenomena of disease, as actually observed, present themselves for consideration. This is a species of pretension, *primâ facie*, not illogical, and hence more likely to be well-founded, than that we have just dismissed. And it actually is better founded; many of the simpler departures from health, and especially many perversions of single functions, may be plausibly explained by the current notions of Physiology: of the latter, Dr. Marshall Hall's beautiful and deeply philosophical system of excito-motory innervation furnishes a striking example. But there is an abrupt limit even to this power. I fear that, in regard of the more complex diseases, Physiology is no more an Œdipus than it, upon inquiry, proved a Cassandra. That Physiology cannot explain the whole sequence of events, and relink, throughout, the broken chain of necessary and contingent circumstances, of any given disease, is unfortunately the veriest of truisms:

(f) This law of accommodation is not only systematically kept out of view (or at least neglected) in the consideration of such ætiological questions as those just referred to, but forgotten in the estimation of already-developed morbid states in generating others. I may perhaps be permitted to refer, for an illustration of the latter point, to observations elsewhere made (*Lancet*, March 17, 1849, p. 280), on the presumed connection between hypertrophy of the heart and cerebral hæmorrhage. Ought this law of accommodation to be lost sight of, when experience shows that certain morbid states, commonly producing (and originally having produced) a given set of symptoms, may actually become necessary to their prevention? M. P. Bérard removed a portion of a large meningeal tumour from a man, who at the time was perfectly free from symptoms of compression of the brain. The moment the morbid mass was cut away, and pressure thus taken off the brain, the ordinary symptoms of compression came on; nor did they disappear till pressure was made with the hand, so as to imitate that exercised by the morbid growth.



still, it enables us, here and there, to seize a causation, and establish a nexus,—no small triumph. We have then, legitimately, hope in store for the future. But what seemingly unfathomable mysteries sleep in the depths of that future! Look to the laws of disease, as determined by observation, and (limited as our determination of these, in proportion to their true number, has been) say, what Physiology shall penetrate the obscurity that encompasses their being. Glance at the class of virus-diseases; what light does Physiology throw on the spread of the virus of syphilis, on the order of its secondary and tertiary phenomena, on the special characters of each? What Physiology makes clear the reason, why the virus of plague should seize on the lymphatic glands, of typhoid fever on the intestinal glands, of influenza on the organs fed by the pneumo-gastric nerve? What Physiology will give demonstrative explanation of the fact, that some morbid changes (as, for instance, calcification of the arteries) affect the body symmetrically? How shall we learn from Physiology why blood-diseases, such as cancer and tubercle—(with their, in some sort, prototypes, chronic poisoning by lead, iodine, spurred rye, &c.)—infect some organs rather than others, some parts of those organs rather than the rest of their substance,—why they fix upon one organ at one period of life, and in one sex, and attack other organs at other periods of life, and in the other sex? Can we hope to ascertain, through Physiology, why ulcerations of the small intestine are almost absolutely limited, among chronic diseases, to the syphilitic and tuberculous diatheses, while cancers may grow in abundance in the frame, and yet the continuity of that bowel remain unharmed? Why, again, inflammations of continuous textures become limited;—why bronchitis, for instance, except under peculiar circumstances, does not pass into pneumonia;—why rheumatic endocarditis, unless in excessively rare cases, stops short at the sygmoid valves, and affects neither the pulmonary artery nor the aorta;—why general peritonitis is a thing almost unknown? What says Physiology of the periodicity of disease—of the nature of constitutional aptitude or pre-disposition to various maladies?

Does it explain very readily why, in a certain proportion of all primiparous women a membraniform plate of bony matter forms between the cranium and dura-mater? But enough; let me not weary you by a multiplicity of such illustrations—their name is Legion. Doubtless, time will give the answers to some of these enigmas; and, doubtless, Physiology will, *in the manners to be presently mentioned*, aid in the search for them. But how greatly more necessary to their probable, and how essential to their positive explanation, is the comparison of a large number of accurately-observed instances of the diseases themselves. Meanwhile, reflecting upon these mysteries, are we not reminded of the poet's phrase,—“Truth is strange, stranger than fiction?” Yes, Gentlemen, laws, as observed, are stranger than principles, as imagined; the realities of Pathology are stranger than its romance.

(c). Thirdly; Physiology may be considered as suggestive of points of inquiry in Pathology, and as establishing a standard of reference for morbid conditions and processes. Now here are, I believe, the true functions of Physiology in regard of Pathology; here the field is peculiarly its own, and here the good it may work out is almost incalculable. On some occasions suggesting points altogether novel for the investigation of the Pathologist, at others giving a new aspect to facts deemed familiar, and in either case lending zest to inquiry; in this sense it is that advances in Physiology must almost of necessity, immediately or more remotely, contribute to advances in Pathology. In this sense it is, that he who labours in the former, is sowing seed which, in due season, shall bear fruit in the latter. In this sense it is, that sound Physiology promotes sound Pathology. In this sense it is, that the hymn to the merits of Physiology can neither be too loud nor too long. In this sense it is, that I shall have repeated occasion in the progress of the Course to draw your attention to our debts, past and positive, future and probable, to physiological science.

Let me here anticipate with a single illustration; it will sufficiently exemplify the manner in which *à priori* and *à posteriori* Pathologists severally avail themselves of discoveries

in Physiology. An acute physiologist, M. Bernard, had been led by experiment to infer, that the production of starch-sugar is constantly going forward in the healthy state in the substance of the liver, quite independently of saccharine or amylaceous diet,—that, in fact, sugar-formation (like bile-formation) is a function of the liver. Subsequently, he discovered that section of the par-vagus immediately arrested this formation of hepatic sugar. Here was material to arouse the attention of pathologists. How, if at all, were these announcements applicable to the pathogeny of saccharine diabetes? Certain Pathologists of the physiological school have already answered the question. In their eyes the mystery hitherto attached to diabetes is gone;—they argue thus: if sugar be naturally produced in the liver through the influence of the par-vagus, that disease clearly consists in simple excess of a natural secretion, and that excess is as obviously dependent on undue stimulation of, and by, the controlling nerve. In a word, saccharine diabetes may henceforth be defined to be a hyper-innervation of the pneumo-gastric nerve. I have heard this said,—statements tantamount to this have been printed. On the other hand, Pathologists of the observation-school proceed more cautiously; they would say, (granting, of course, that M. Bernard's Physiology is sound,) here is a new element of healthy chemico-vital acts, in its general signification deeply important, and having some probable, or at least possible, connection with diabetes. Here, too, is evidence of the necessity, in all cases of that disease, for the clinical consideration of the pneumo-gastric nerve. But they would not attempt, prior to experience, to draw any positive conclusion as to the part (if any) played by that nerve in generating the malady. They would argue (the healthy influence of the nerve being fully admitted): it may be, that diabetes depends on hyper-stimulation or perverted action of the nerve; or it may be, that it depends on disease of the connected portions of brain; or it may be, that the undue accumulation of sugar in the blood is a consequence of a primary change in the liver itself, whereby it yields an excess of sugar to a natural amount of nervous influence; or it may

be, that the blood circulating in the liver is in such a state as to lend itself with unnatural facility to sugar-formation; or it may be, that the nerve, the liver, and the blood, are natural in regard of the sugar-producing force, but that, from some defect in respiration, the sugar formed is not oxidised in the lungs with sufficient ease and rapidity. They would maintain, that observation alone could decide which, if any, of these hypotheses was correct; and they would regard M. Bernard's results as having furnished a new standard, and suggested a new path of inquiry, but not as having determined the nature of diabetes, prior to experience.

§ IV. If the legitimate application of Physiology be thus limited, why, it may be asked, has the attempt to found Pathology on Physiology, prior to experience, always proved, both with authors and readers, teachers and disciples, so singularly popular? Nine conditions occur to me as having mainly operated in this direction.

An obvious cause of the popularity the system has uniformly enjoyed, is the fact of its saving trouble. There is, probably, no more laborious intellectual employment in existence, than the effort to work out, by clinical observation and interpretation, the laws of any given disease. It requires a combination of powers of physical and moral endurance and mental capacity rarely meted out to the same individual. Constituted, in truth, as man is, by nature an indolent being, is it to be wondered that when presented with "first principles," whereby he is empowered to fix the origin, course, and issue of diseases; and, independently of direct labour of any kind, but simply through a musing, *dolce far niente*-like exercise of his reasoning faculties, to mould them into such forms as may best suit his purposes,—is it to be wondered, that he seizes with avidity on the complacent instruments, and eschews with determination the severe and rugged toil of direct observation?

But, secondly, not only do these principles save labour to the original investigator, but to the learner. They are a precious and a facile *Memoria Technica*. Few memories can retain an accumulation of generalised facts or laws, no matter



how interesting or important they may be, unless by constant exercise and perpetual reference, either simply mental, or direct, to the objects referred to by those laws. Here, then, is labour again involved. But, with a supply of physiological "first principles" at command, the dependent facts of pathology string themselves in readiest order; or, if the memory be hopelessly sluggish, the principles are there to reproduce the facts at will. The only drawback is, that as the solidity of the principles, and hence of the facts, owing them pater-nity, may be questioned, the advantage of readily recalling or reproducing these is more than problematical.

But physiological principles serve, it is argued, as guides, prior to experience, to the determination of unknown pathological conditions; and hence a third source of their popularity. To their *investigation*, yes; to their *determination*, no; observation must intervene. In fact, the argument, though just in foundation, is a sophism in its application. That in a new and difficult path a guide is an admirable thing, is a position, abstractedly stated, which few will dispute. But the utility of any particular guide must, as of all other things, depend on its fitness or unfitness for its purpose. Suppose yourselves travelling in a new country, and desirous of ascending a mountain peak, at all times difficult of approach, but the ascent of which has by land-slips, changed course of torrents and avalanche-falls, been recently rendered a new and more complicated problem. Guides present themselves. They may be of three kinds. Men may crave to lead you, who honestly confess they have not been the way since the occurrence of the changes. But they assure you emphatically, that, from their acquaintance with the natural conditions of the mountain passes, the course of the rivers, the tracks by which avalanches *must* fall, and so forth, they can form a complete and perfect notion of the changed conditions they have never seen. They can deduce the possible Pathology of the passes from their Physiology. You probably pay such persons as these a compliment on their sagacity, (as estimated by themselves,) but decline to place your flesh and bones at their disposal. Or a second group-

may solicit the honour of conducting you ; and, upon inquiry, you learn that these aspirants have not only not explored the altered mountain routes, but have settled among themselves to guide you on a plan that (under circumstances of mountain convulsion, more or less similar,) has proved over and over again a fallacious one. You pronounce them to be impostors, and suspect their motives. You feel that the guidance offered by both these groups is shadow, not substance,—that they have made no further advance than yourselves to the desired haven ; you feel that in common with yourselves they have the laborious path of experience to tread. But a third group of guides may offer, who, as before, so likewise since the changes, have made the ascent many a time and oft,—men to whom each step almost in the path is by experience familiar. To the superiority of these you bow ; to them you confide yourselves in all security ; they are guides not alone in name, but in reality.

And so, in the difficult and untrodden routes of Pathology, three classes will propose to guide you in the determination of new conditions. Men who have no experience of the regions in which they purpose to lead, but who believe honestly in the perfect aptitude of a new species of theory to serve in lieu thereof ; men who are not only devoid of that express experience, but who, in defiance of past example and warning, would guide you by a worn-out species of theory, to inevitable error ; and men who, having that experience, know the route to truth. And of the former false guides in Pathology, or principles prior to experience, those rising with largest and most hopeful promise, have ever set behind the most hopeless failure. Soaring brightly aloft like rockets, they have fallen darkly like their sticks. Archæism, Pneumatism, Animism, Stahlism, Hoffmanism, Cullenism, Brownism, Contra-stimulism, Broussaisism ! Ye guides of the hour ; ye blind that led the blind for a season ! say, even now, before the personal memory of some of your authors has passed away, say what verdict does posterity pronounce on the quality of your guidance ? Alas, posterity is pitiless and stern ; it thinks naught of the genius and ambition and



positive services of your authors,—it simply proclaims that you arrested all progress in true knowledge for the time, and in the quarters your guidance was trusted to !

Fourthly ; Physiology is very constantly assigned the merit of establishing pathological facts and doctrines, which in reality it has had no share in substantiating. An apt illustration occurs in a recent work, by one of the first physicians of the day, whom I am proud to call my friend, and whose admirable style almost redeems (what appears to me) the oversight in the thought,—as choice setting gives somewhat of the value of the gem to the imitation. He observes :—“ Every physiological physician knows well that a morbid condition, which may have been months or years in forming, can only be effectually and permanently removed by means which act slowly and for a length of time, not on one part only, but, more or less, on the whole system.”(g) Now, the general doctrine here broached (although exceptions might readily be cited) is in accordance with observation. But, obviously, the credit of establishing that doctrine should be assigned to pathological, not physiological, observers. What physiological facts are known, justifying the assertion, even as a probability, prior to experience, that morbid conditions, slowly built up, can only be slowly thrown down ? All that is positively known of the rapidity with which waste and change of tissue are physiologically effected, would warrant the expectation of sudden removal of long-existent disordered states being easy of accomplishment, when once the right impulse was given. But pathological observation has proved that (in the present state of therapeutics, at least,) the reverse is the rule ; while the laws regulating exceptional cases furnish a very curious subject of inquiry.

So, too, it is the habit to class the now popular investigations into the morbid conditions of the blood with physiological researches, though their distinguished re-originator, Andral, gave the hint of their true character in the terms “ *Pathological Hæmatology*.”

(g) The Physician's Holiday, (p. 268.) By John Forbes, M.D., &c. &c. A work abounding in varied beauties of thought and expression.

Again, somewhat similarly, a large proportion of experiments on living animals, essentially pathological in their character and aim (as, for instance, those consisting in the injection of various noxious substances into the blood-vessels,) are confounded, under the general name of physiological, with others really deserving this title. Now, the effect of this misnomer is, that when perchance the former class of experiments leads to suggestions available in the direct investigation of disease, Physiology, instead of experimental pathology, unfairly receives the credit.

Fifthly ; an artifice very commonly resorted to by Physiological Pathologists, has contributed not a little to throw a false glare of wisdom over their *à priori* modes of divining the conditions of morbid phenomena. From alleged speculative examination of certain groups of physiological facts, they set forth a certain principle of pathological action. The principle created and announced—let us look around, say they, for illustrations ; and, behold, these are not long in coming. Who can feel disposed to scrutinize sharply the logical quality of a system that leads to such startling results ? Who is not rather disposed to bend the head irreflectively before an intellectual procedure of such seemingly high stamp ? Observe what it would signify. A man, from the workings of his brain on actions of *one kind*, creates principles that must regulate actions of *another kind*,—and finds in the ready illustrations that present themselves, the proof that Nature has precisely the same views on the matter as himself,—that, in other words, he is possessed, *quoad* intellect, of creative faculty. Beautiful and wondrous indeed would the process be, were it only honest and true ! But has it these qualities ? Divest yourselves of the obsequious spirit of admiration, and assume the cold judgment of the critic, and I fear you will find that it has not. You will, I think, discover that in such cases the real course of proceeding has been this. A single fact has been caught up, of a novel or ill-appreciated kind ; that fact has been generalized at once ; and this generalization is the putative *à priori* principle, the *quasi* offspring of a creative faculty. So that, if

this anatomy of the pseudo-creation be correct, the whole process resolves itself into elements of the following species ; a fact due to observation ; an inference from this, belonging to a very vicious class of sophism, the *argumentum à particulari ad universale* ; and, lastly, a conversion of this imperfect *à posteriori* inference into a baseless *à priori* principle. Is it not plain that the process sins logically ? Does it not strangely remind one too of a sleight-of-hand *coup*,—a sort of intellectual legerdemain ? Quick, presto ! it is done,—your fact is a principle !

Sixthly ; more crafty still are they who throw off *à priori* applications of Physiology to Pathology, and look not for illustrations, and care not for acceptance at the moment. Time, they mysteriously hint, will judge them. As the world advances in observed wisdom, facts will come to demonstrate that the Deity has given to some men intuitive prescience. Well, time flows on : the oracles are forgotten. But at length, perchance, some observer, in the course of investigations, wholly without reference to, or knowledge of, the bygone divinations—on grounds completely different, with aids as dissimilar, through philosophy egregiously opposed,—comes to results in some sort according with, or corroborative of, the oracles that have slumbered. Then comes the *cantus triumphalis* of the augur. Behold ! I said it—I divined it—years before your dull observation had started even the terms of the problem, my principles had worked out the solution. And with lusty voice the crowd sings chorus. But is this right ? Ought the crowd to lend their harmony of praise ? Is the seeming merit all real ? Is there no artifice here also ? Does the crowd remember, did it ever know, the multitude of instances in which similar divinations have been made, have failed, and have slept the sleep of death ? Must not the crowd, when reminded of these, admit that the triumph is signally like that of the professional fortune-teller, who, wrong fifty times, comes right, she knows not why or wherefore, the fifty-first ? And lo ! a present miracle, exclaims the gentle candidate for a “ fair or a dark man ;” while the philosopher aside knows that an occasional successful hit is a matter

of necessity, the frequency of which may be determined by mathematical calculation. No; it is not he who starts possibilities, or even probabilities, on illogical grounds, but he who by logical inference converts possibilities into certainties (positive or negative), that, unless I deeply err, deserves the homage of his fellows.

Seventhly; people are dazzled by the wondrous, almost miraculous, appearance of progress in Pathology, which the direct application of physiological speculation occasionally effects. Chemical Physiology, which has of late taken the lead in this point of view, deserves particular notice here. It seems to have taken that lead; first, because one or two chemists (men of singular mental power, and like the Chemi-Iatrists of old, chemically in advance of their age,) gave the impulse; secondly, because, it has all the charms of novelty; and thirdly, a section of Pathologists seems to seize upon it, because (singular contradiction!) they are more or less unacquainted with the qualities of its alleged facts. Accepting these facts from others, unconcerned in reputation by the correctness or incorrectness of these facts, blind to the difficulties of their establishment, uncognizant of the host of uncertainties pervading the very atmosphere wherein they originate, this section of Pathologists manages these facts (or pseudo-facts) in the beatitude of innocence; and the man who, as a clinical observer, would hesitate to pronounce positively on the characters of a symptom or a physical morbid change, where any source of fallacy existed, although he had examined it scores of times perhaps, welcomes, as absolute truths, chemical inferences essentially plunged in obscurity and doubt. His conscience is at ease, and away he works. And while no complete or trustworthy analysis of a single morbid product, in accordance even with the nascent organic chemistry of the present day, exists; while masses of matter are thrown on one side under the title of extractives, or, as one might baptise them, the "great unknown;" while the very constitution of protein is matter of dispute; while one of the first organic chemists of the age (Dr. Prout) "knows at present of no apparatus or means of operating, capable,



where azote is concerned, of unequivocally deciding about the presence or absence of *one proportion* of hydrogen or even of oxygen in a complicated body," (on Stomach, &c., 4th edit., p. 556,) and "is satisfied that very few, if any, of the formulæ now so much in fashion with chemists represent the true constitution of organized substances" (Dr. Prout: Preface, p. viii.); while the alkaline reaction of the blood, even, was yesterday referred to one salt, is to-day traced to another, and will to-morrow, in all probability, be affiliated to a third; while we know, that of certain substances entering into the blood by absorption, all chemical traces may be almost instantaneously lost, though, when the same substances are mixed with dead blood without the body, those traces may be readily followed; while all these evidences of imperfection (and they are but a brick of the Babel) stand forth in warning reality, men are found to pretend that Pathology is to be based on chemistry, prior to experience, and that the votaries of eclectic observation are to bow to its dictates! Here appear men, who (just as if asked for a piece of Prussian-blue,) will most readily undertake to make pus, or cancer, or tubercle, at a moment's warning, from any tissue-detritus or exudation you please,—all they want is the temporary loan of an atom or two of oxygen, or hydrogen, or nitrogen, as the case may be. No, Gentlemen, place no confidence in these figments of an over-heated fancy. Even if they seem to accord with the results of experience, they are only possibly, not necessarily just; if they fail to accord with those results, they are necessarily inapplicable for pathological purposes, if not necessarily unsound. Besides, it is not the higher order of chemists who produce, or encourage the production of, these fanciful syntheses. Did *they* profess that such applications of chemistry were sound and just, it would be hazardous to contest their scientific quality. Still, some chemists of most brilliant repute have promulgated chemical theories of diseases, unchecked by observation; and so given, indirectly, a colour of justification to the wild exploits just referred to. As a single instance, let me offer you Professor Liebig's hyper-oxidation theory of phthisis,—striking, simple,

plausible, attractive, in seeming vastly progressive, wanting nothing but the confirmation of clinical observation,—but when submitted to that test found, as is now known, to clash with obvious facts, and therefore (whatever excellence it possesses as a piece of profound chemical inference) inapplicable to, and inadmissible as a portion of, the natural history of disease.

Eighthly ; the tendency to found Pathology directly on Physiology may be traced to the apparent plausibility of the attempt. Physiology studies the actions of the frame in health ; Pathology studies the actions of the frame in disease. The knowledge of the discords an instrument is liable to emit, and the modes of correcting these, are matters of direct inference from the knowledge of its concords ; and, similarly, Pathology must be matter of direct inference from Physiology. But, after all, here is nothing more than a specious sophism. Speaking abstractedly, it may be perfectly true, (and absolutely must be so,) that pathological action, no matter how complex, is a derivation from physiological action, simple or complex ; but the question is to be considered practically. Now we have already seen, if the statements of fact I have made be just, how little can actually be ascertained of the laws of disease by this method ; and if you consider, for a moment, what a human being is in his physiological actions, you will hardly wonder that it should be so. Look upon one of the most elementary chemico-vital series of acts we know of—the birth, life, and death of a secreting cell. Consider the myriads of such cells concerned in the production of but a few drops of secretion ; conceive them, at their various periods of healthy existence, influencing, as they must, physically, chemically, and vitally, not only the locality they occupy, but (through the blood and nerves) parts more or less remote ; and say, what can we predicate with safety of the precise nature of those influences ?—what of the re-actions which those influences excite ? And yet we would presume to lay down, prior to experience, the effects of those influences when deranged,—and not only when one set of such influences, but when multitudes of them, are deranged,—when acts



of a complexity beyond conception err, acting, re-acting, advancing, retroceding, yielding, clashing, in endless varieties of confused combination. Alas for him that arrogates here the gift of divination, and fancies he can, through Physiology, forejudge the issue of the mishaps of even a single set of secreting cells ! Lending himself prescience, he is really no more cognizant of God's providence in this matter, prior to experience, than the sauntering school-boy who, throwing stones in the stream, dreams not what effects the motion of the ripples he idly watches, as they circle to the bank, produce through the atmosphere of the earth. (h)

Lastly : an impression appears very prevalent that the direct application of Physiology (physical, chemical, or vital) to Pathology is peculiarly philosophical or "scientific;" while the direct observation and interpretation of the phenomena of disease scarcely possess a claim to that dignified title. Hence a ninth source of the popularity of the former. Louis, for instance, from the sustained critical observation of years, accumulated a series of individual facts, whence he induced a natural history of phthisis, finally establishing that history in regard of the points, and under the circumstances, examined. But he ventured not even on an hypothesis as to the essence and pathogeny of the disease. On the other hand, an eminent chemist, studying the laws of the oxydation of tissue in general, passed through a series of analogical inferences, treated them as if they were established truths, and propounded the theory of the disease already alluded to, without professing ever to have observed it. Now I have repeatedly heard it said that the treatise of Louis had certainly great

(h) I cannot resist the pleasure of justifying this illustration by an extract from the writings of a man who possesses, in the rarest degree, the art of throwing the character of poetry over the profoundest truths of science—the inventor of the Calculating Engine:—"The waves of the air, thus raised, (by the human voice in speaking,) perambulate the earth and ocean's surface, and, in less than twenty hours, every atom of its atmosphere takes up the altered movement due to that infinitesimal portion of the primitive motion, which has been conveyed to it through countless channels, and which must continue to influence its path throughout its future existence."—*Babbage : Ninth Bridgewater Treatise*, p. 110.

merit, as a work of observation ; but that its author was not a man of science ; while the great chemist's science was so profound, so expansive, and so ductile, that he might well dispense with observation. When it was urged, in reply, that the conclusions of Louis were solid and unimpregnable, —time-marks of progress in man's knowledge of disease,—while the hypothesis of the great chemist flashed brightly for an hour and was gone,—the rejoinder was still the same, heads were shaken, and the chemist was still pronounced, *in regard of this matter*, the man of science.

What, then, is science, and what constitutes a science ? I cannot, for my part, conceive a science to be anything more or less than the sum-total of the established generalizations belonging to any particular kind of phenomena, classified according to their resemblances and their differences, their attractions and their repulsions ; generalizations which, in turn, cannot be formed from any possible data, but from the comparison of accumulated individual phenomena of that kind. (i) It seems logically impossible to imagine any other mode of formation of a science. Now, if this be true, all facts and generalizations belonging to sciences, collateral to that under investigation, no matter how close their relationship, can form no part of itself, as *matter of necessity*. And their direct transference from the one to the other—the attempt to introduce them from the one into the other as *part and parcel of itself prior to experience*—is a flagrant breach of logic,—a proceeding based on the assumption, that, because a thing is possible or likely, it is actual and real. It matters not how perfectly just any given principle is in connection with facts of its own order, and how fully it answers the conditions of a scientific law,—the moment it is transferred, and applied in the guise of an established principle, to facts of a different, though allied order, as great a breach of logic is committed (and how can science exist, where logic is wanting ?) as if the principle were divested of all claim, in its original position, to scientific character. If this be true, and I think it would be

(i) Reference is of course made especially to *natural science* ; but the statement given will, I believe, apply fully to *abstract science* also.

difficult to prove it false, it is he who observes, interprets, compares, and classifies facts and laws of any particular order, and not he who makes direct application of inferences from facts of one order to facts of another order, that is the true labourer in the cause of science. And let it not be supposed that, because Physiology (chemical as vital) will advance as time flows on, that its claims or powers to form Pathology prior to experience, will ever change. Pathology, too, will advance, both independently, as it has hitherto done, by pure observation, and, dependently, in company with Physiology and the other collateral sciences, through the suggestions they offer, but not through the *à priori* principles they are made to force upon it. The mode of searching after truth can never change; what is real now in the philosophy of establishing a science will be true to all eternity.

To illustrate this argument, and give greater precision to its bearing. Let it be supposed that any given chemical law has been established by observation in regard of certain healthy actions. That law constitutes, henceforth, an element of the science of physiological chemistry. But it may, or may not, hold in regard of those actions when diseased. It is for observation to determine this. The physiological law is merely *a candidate for a place* in the science of Pathology, to be accorded or denied it by observation. Applied directly from Physiology to Pathology, it degenerates from a law into an hypothesis; and no hypothesis can ever form an actual part of a science. On the other hand, hypotheses are never to be disdained; like servants, of whose character we are doubtful, they are to be taken on trial. Of hypotheses I would say, *accept none; reject none; try all*,—provided only they do not distinctly clash with any absolute law of natural or abstract science. But, in making this admission, I am desirous of not being supposed to imply that the mere habit of starting loose conjectures is in my mind either useful or commendable. I consider it, on the contrary, a sufficiently vicious practice, diverting men's minds (if it exercise any effect at all) from the true sources of knowledge. It has become very much the habit at the present day, when objections are made to the

hap-hazard system of chemico-physiological conjecture, to find them met with observations, something in this wise:—"What, you would denounce hypothesis! you forget, my good sir, that the existence of America was an hypothesis in Columbus's brain, before he proved its truth by experiment." As if every man who started a conjecture, was, *ergo*, a Columbus; as if every conjecture contained an America! (k)

But, to return;—not only is there in many quarters a tendency to overrate the philosophical quality of direct introductions of the principles of one natural science into another, but a persuasion of the actual and essential inferiority of observation as an intellectual instrument. One notion, which contributes to this low estimate of observation is, that the principles of the physico-mathematical sciences were established without its aid, and are, in fact, either pure mental abstractions or mathematical results. But this is completely erroneous. In physico-mathematical astronomy, observation lies at the root of all acquired truths. "No one can doubt," says Mr. Whewell, "that in historical fact the laws of motion were collected from experience. That such is the case is no matter of conjecture. We know the time, the persons, the circumstances belonging to each step of each discovery." (l) Kepler's laws of planetary motion were primarily not ascertained even by induction, but by simple direct observation. We know from Kepler himself that, "before hitting upon the conception of an ellipse, he tried nineteen other imaginary paths, which, *finding them inconsistent with the observations*, he was obliged to reject." (m) Yet further, even the axioms of geometry are found, in ultimate analysis, to be experimental truths, generalizations from observation. Nay more, as shown by Mr. Mill, "the science of number is no exception to the conclusion that the processes even of deductive sciences

(k) The illustration is otherwise singularly inapt. Columbus was led by a system of inductive reasoning to his wondrous discovery; and had it been possible for him to make the experiment, before announcing the expected result, it is evident he would have done so,—the quality of his intellect was eminently Baconian.

(l) *Philosophy of Inductive Sciences* (i. p. 238).

(m) *Mill's Logic* (i. p. 363).



are altogether inductive, and that their first principles are generalizations from experience.”(n)

Rest satisfied, too, Gentlemen, that observation is not in our science the humble drudge's toil, which the ante-experience school of Pathologists would persuade you to believe it. It is a sensual and intellectual process of the deepest and most varied difficulty. Remember (to quote the admirable words of one who has gained a commanding place in the philosophy of abstract science, Mr. Mill), “the observer is not he who merely sees the thing which is before his eyes, but he who sees what parts that thing is composed of. To do this well is a rare talent. One person, from inattention or attending only in the wrong place, overlooks half of what he sees; another sets down much more than he sees, confounding it with what he imagines, or what he infers; another takes note of the kind of all the circumstances, but being inexpert in estimating their degree, leaves the quantity of each vague and uncertain; another sees, indeed, the whole, but makes such an awkward division of it into parts, throwing things into one mass, which require to be separated, and separating others which might more conveniently be considered as one, that the result is much the same, sometimes even worse, than if no analysis had been attempted at all.”(o) Ponder, too, on these striking words of Sir Gilbert Blane:—“The truth seems to be, that a higher order of intellect, a more rare and happy genius, a more correct and better-tutored understanding, is required to elicit practical truths by observation than to coin theories.”(p)

§ V. To conclude, Gentlemen, my convictions on the main question, on which I have just addressed you, amount concisely to this:—That Physiology (physical, chemical and vital) cannot logically, nor hence scientifically, be used for the *pre-determination* of the conditions and laws of Pathology;—that Physiology may logically, and hence scientifically (if excessive caution and judgment be exercised) be directed, subject to

(n) Mill's Logic (i. p. 336).

(o) Mill's Logic (i. p. 438).

(p) Elements of Medical Logic.

the control of experience, to the *explanation* of those conditions and laws, when established by observation ; (q)—and that the essential function of Physiology in regard of Pathology is *measurement and suggestion*,—the supply of a standard of reference and the suggestion of points and courses of inquiry : while, on the other hand, on those Tablets (r) are set down the sole *direct formative processes* of the science. These convictions I have acquired deliberately, and hold firmly ; and I have ventured to express them before you with earnestness.

(q) Any one who will reflect, without prejudice, on the past history of medicine will, I think, be disposed to agree with me, that the passion for *universal and rash* explanation (the habit of looking for the *why*, and not the *what*, of morbid phenomena) has indirectly proved one of the most fruitful sources of its slow progress ; an unsound explanation, once accepted, interferes, to an indefinite time, with the discovery of the true one. Man feels within him, undoubtedly, a sort of instinct of causality ; but it does not follow that he is always justified in obeying it. It is often one of the nicest problems to determine whether, under certain given circumstances, the effort at explanation is or is not logically legitimate. It may be that a mental triumph is achieved by resisting that instinct, just as a moral triumph is achieved by a successful struggle against the baser animal propensities. Now, one essential to the admission of an explanation appears to me to be, that this should flow easily (as, of course, logically) from the premises, “The quality of *Truth* is not strained.” The amount of effort expended, and of ingenuity displayed in framing any given explanation of obscure pathological existences is very frequently in the direct ratio of its fallacy. Instead of worshipping such ingenuity, as is the fashion, there would be deeper wisdom, I cannot help thinking, in regarding it, *quoad* results, as fraught with illusion and mischief,—as a promoter of spurious, and, *pro tanto*, a preventive of true knowledge. I would say to a youth, starting on the journey of scientific life,—whenever you have woven a web of explanatory theory, peculiarly delicate and subtle, and peculiarly gratifying to your *amour propre* in the matter of invention, deeply suspect your correctness,—nay, sacrifice the product of your brain at once, unless you know yourself to be possessed of that higher moral courage, which will enable you, after it has been given to the world, to surrender it without struggle, if proved unsound. If you fail to do this, the chances are strong that your theory becomes your intellectual idol, turning you from the just apprehension of truth, even to the end of your existence, blunting your senses, and perverting your judgments. Such was the effect on his own intellectual being (and this was one of no common order) of Broussais’ invention of the theory of Irritation. Cullen had the sense and courage to teach and practise the frequent sacrifice of his idol-theory of spasm ; otherwise the memory of that distinguished man would have been but the reflection of error. And Cullen, in this partial relinquishment, stands almost alone.

(r) Vide p. 5.



In stating them, I have been obliged to trench somewhat on the departments of Chemistry and Physiology. But it is gratifying to me to believe that I have not broached opinions which (as far as I can gather from the tone of their published writings) clash with the doctrines of those who, in the highest degree, represent those departments here and elsewhere. It is not, I believe, the acknowledged lords of the crucible and alembic, nor is it they who have peered most closely and most steadily into the mysteries of healthy life -- it is not the Berzeliuses and the Grahams, the Müllers and the Sharpeys,—that will cry, Hold! because an attempt is made to set a limit to the illogical intrusion of their sciences into the domain of Pathology. . . . But, whatever my views, I do not the less entertain personal regard and esteem for men who, urged by other views, conscientiously acquired, labour, in accordance with these, to widen the confines of our science and our art. Somewhat as, in the moral world, we visit with our reprobation the crime, rather than the criminal, so, in the intellectual, we may denounce what we conceive to be false in logic, while we cherish him whom we hold to be a pseudo-logician. In this spirit I hope myself to be judged. So long as we are all animated with the love of, and yearning after, truth; so long as we spare no toil to achieve it, our feelings for each other can be none other than of mutual regard and good brotherhood. No matter how different the paths which the various qualities of our minds, our physical constitutions, and contingent circumstances may lead us to pursue, we then all agree in this,—we are toilers after truth for the benefit of mankind. It is this very toiling after truth (the aspiration even more than the possession) that gives the stamp of dignity to our pursuit, and elevates the medical profession in the scale of humanity.

And herein lies one of the noblest attractions to that profession,—an attraction as little tainted with selfishness, (at least of a low order,) as, perhaps, any human motive. It is a profession on which worldly honours and lucrative posts are not showered. It is a profession to which even the homage of lay respect is not uniformly conceded; men of education,

men holding seats in the Senate of the land, are found to utter scoffing platitudes at our expense, because, forsooth, we are not omniscient,—the senatorial benches, they are in the habit of frequenting, doubtless teeming in the quality! Charlatanism, decked gorgeously in its unholy spoils, walks abroad in mockery of our science; and there is none to say it, Nay. Our veterans in the army are refused medals; our youths in the navy are denied the decencies due to their social standing; and (climax of incomprehensible indignity!) the Government declares us incompetent to judge and to guide in matters belonging to our own province. It is determined the Public Health shall be Legislatively cared for; and forthwith persons (with a single exception) whose days have been passed in pursuits wholly unconnected with disease—men unversed in even the veriest rudiments of the science of life—are selected as the fittest instruments for dealing with the most intricate questions of practical hygienics,—as though incapacity were the father of usefulness!

Well, be it so! Notwithstanding all this, pursue the even tenor of your way, unruffled by these crosses. Dwell on the memory of that brighter side of the picture, I, a moment since, held before you. Remember, too, you have the reward of moral distinction, and of scientific reputation, it may be, fame, within your reach. Scarce shall your toil have added a new fact or a new inference to our science, before it goes forth to the ends of the earth, and your names may be marked *ἐνεργέτης* in furthest regions. Your labour has borne fruit for the benefit of universal man, “no matter what colour an Indian or an African sun may have burned upon him;” your labour has united the families of the earth in the link of common obligation. Brilliant and soul-stirring reward!

That remarkable man, whose posthumous Memoirs are now appearing, clothed in a mystical solemnity, according with their fabled place of issue, “beyond the tomb,”—he who passed through well-nigh every grade of social condition, at one time a vagrant outcast, at another ambassador from his own to the chief courts of Europe, and who, in his experience of the affections and passions, the vanities and ambi-

tions, struck every chord of the lyre—Chateaubriand, I mean—tells mankind, that life offers but two real goods; religion and love. Yet would I plead for the reality of another, and that other not beyond *your* grasp,—the steadfast pursuit of true knowledge, and, through this, the ennobling consciousness of effort to aid in fulfilling the mission assigned your profession by the immortal Descartes, when he said, (and no Government can blot out the words,) “If the human species can be rendered perfect, the means of that perfection are to be sought for in Medical Science. (s)

(s) The phrase runs in the original:—“Si l’espèce humaine peut être perfectionnée, c’est dans la Médecine qu’il faut en chercher les moyens.”

